

Review: The impact of Saharan Air Layers on atmospheric stability and clouds during the BOWTIE campaign

By: Anna Trosits et al.

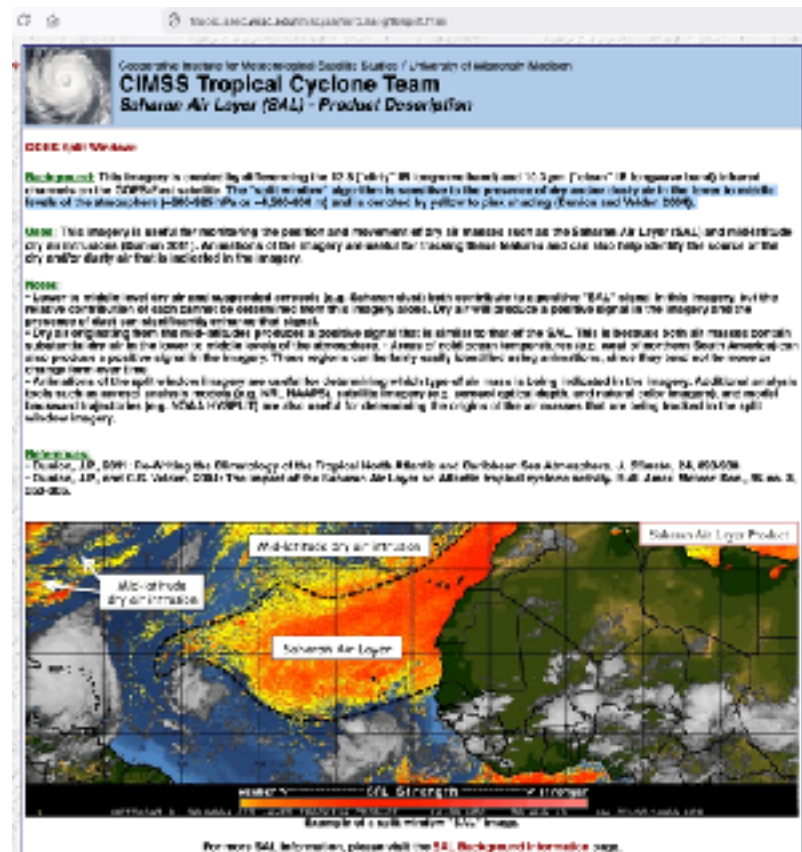
Convection over the tropical Atlantic is frequently coinciding with Saharan Air Layers and alters deep convection (e.g. Wong and Dessler (2005)) and shallow convection (e.g. Narenpitak et al. (2023)). The observations described and analysed in this study from the BOWTIE campaign support previous studies and provide a higher resolution, particularly in the vertical than previous studies have analysed in this context.

Unfortunately, the manuscript includes a large amount of references that are out of context, are not supportive and mixed up. The authors state that artificial intelligence tools have been used to support “the literature research by finding relevant literature”, but do not appear to have checked the results. This becomes particularly apparent by including a colorscale description in a sentence that can be found nearly identical online.

Based on these findings the manuscript currently does not gain my trust and I recommend rejection of the manuscript. All references need to be thoroughly checked and algorithms, products and results correctly described and interpreted.

Major concerns:

I. 193f: The algorithm description is nearly identical to https://tropic.ssec.wisc.edu/misc/sal/info_sal_g16split.html and including the colorbar description “denoted by yellow pink shading”. While in the next sentence the manuscript states “the pink-white shading”, presumably still referring to the same plot and quantity, the dust layer.



References:

I. 28: Nicholson, 2018 focuses on the African landmass, where the term ITCZ is highly controversial. It does not support the claim here.

I. 41: Foltz and McPhaden, 2010 investigate the ocean dynamics of Atlantic meridional and Nino modes and is not about clouds. Foltz et al. 2019 is also not a very fitting citation here as it has a strong focus on the ocean rather than the atmosphere.

I. 91: please explain in more detail what is meant by “ERA5-based retrievals”, as Schnitt et al. (2024) use ERA5 quantities only for validation.

I. 98: Wrong reference. Should be Acquistapace et al. 2022 (<https://doi.org/10.5194/essd-14-33-2022>)

I.139f.: Is IER really from IWC and lidar extinction? In the cited papers and the clounetpy software I can only find radar products for IER.

I. 250: Gutleben et al. (2019b) shows no “enhanced heating at the ocean surface”. It shows a stronger net cooling in the marine boundary layer. How a “heating at the ocean surface” “leads to lower tropospheric stabilisation” is not clear to me.

I. 290: How does Yang et al., 2013 support this claim. Yang et al., 2013 is about dust sedimentation and not about convection.

At various locations in the manuscript Wong and Dessler, 2005 is cited. That study is about the impact of SAL on deep convection with a brightness temperature of 250K, much higher than the low and mid-level clouds studied here.

I. 302: “CuCon clouds decreases in the presence of Saharan dust [...], which is in agreement with Wong and Dessler (2005)” does not hold. Wong and Dessler (2005) looked at deep convection with cloud tops way below freezing, according to the table in the supplement in this manuscript “CuCon” have CTH below the freezing level.

Tab1.: max vertical range of 14km does not agree with the sounding profile in Fig. A3 which goes already to 18km.

Minor comments/typos:

throughout: the term “alto clouds” is rarely used in the scientific literature. Please use “mid-level clouds” or another common term.

I. 27: “direction and strength of convergence”

I. 59: unrendered reference

l. 114 + l. 191f: these long URLs disrupts the reading flow quite a bit and might also not persist. Could these figures with the URL as reference be added as supplement?

Fig. 4: The lower bar indicating the dust cases and no-dust periods includes some white areas (Sept 3 - Sept 21) which are explained in l. 260ff, but it would help to state here in the caption as well that these are cases are discarded from the direct comparison of SAL/no-SAL.

l. 323f.: You mention that “enhanced occurrence of alto clouds in study periods only occurs AT THE TOP of the SAL”, but looking at e.g. case 3 on Aug 31, the mid-level clouds are even above the SAL and not visibly connected. This does not apply to all cases, but it is a nuance that should be mentioned. Did the authors consider running backtrajectories at the mid-level cloud height or analyse satellite images to understand the origin of these mid-level clouds and whether or not they are originating from outside the SAL?